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THE MEASUREMENT OF CONFIDENCE AND TRUST

TECHNICAL DOCUMENTARY REPORT NO. ESD-TDR-65-299

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AIR FORCE SYSTEMS COMMAND
UNITED STATES AIR FORCE
L. G. Hanscom Field, Bedford, Massachusetts



Project 7682, Task 768203

ESRH

(Prepared under Contract No. AF 19 (628)-2450 by
Tufts University, Medford, Massachusetts.)

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FOREWORD

This report was prepared by Tufts University, Psychology Department, Medford, Massachusetts, under Air Force Contract No. AF19(628)-2450 in support of Project 7682, Task 768203, and in part by Contract No. 494(15) with the Office of Naval Research. The Contract Monitor is Dr. Elizabeth H. Nicol of the Decision Techniques Division, Decision Sciences Laboratory, Electronic Systems Division, Air Force Systems Command.

This Technical Report has been reviewed and is approved.


DONALD W. CONNOLLY
Project Officer
Decision Sciences Laboratory

ROY MORGAN, Colonel, USAF
Director,
Decision Sciences Laboratory

THE MEASUREMENT OF CONFIDENCE AND TRUST

Abstract

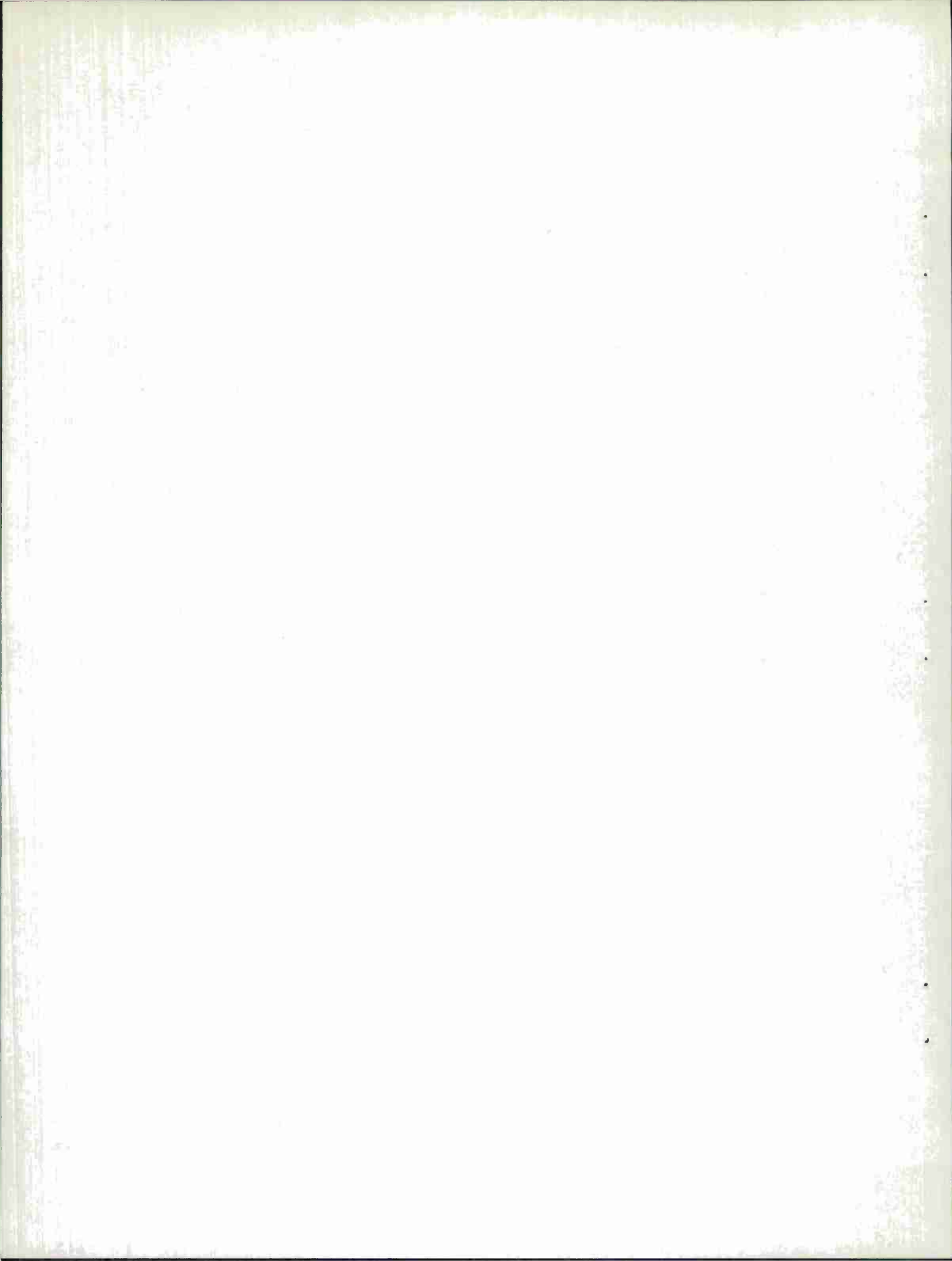
This report is concerned with the development of a research methodology and a theoretical framework for investigating the effects of social influence in a simple judgmental situation. The laboratory task entails a simple binary judgment as to whether a displayed angle departs from 90°; before making his own response the subject is provided with the answer of a hypothetical partner, programmed at a certain fixed accuracy level. The responses are made in terms of a special betting scheme which penalizes the subject for overstating or understating his confidence. The two main experimental variables in this study are the difficulty of the discrimination and the announced reliability of the hypothetical partner. Theoretical predictions as to the effects of these variables on the relative value of confidence measures are confirmed. However, further methodological development is required to increase the realism of subjects' confidence scores.

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The Measurement of Confidence and Trust

T. B. Roby
Tufts University

Teresa Carterette
Simmons College

Introduction

The effects of social influence upon perception and behavior constitute an important and rewarding subject for social psychological research. The experimental literature contains a number of dramatic and reproducible demonstrations of the fact that such effects occur (e. g. Sherif, Asch). In order to go beyond these pioneer studies however, it will be necessary to obtain precise information as to the factors that determine the extent of the influence effects. This objective entails extensive methodological development both in the specification of experimental variables to be independently manipulated and in the measurement of dependent variables indicating influence.

The present line of investigation grew out of exploratory studies of the acquisition and exchange of information in small group performance (Roby, Harleston, and Eyde, 1961; Farrell, Nicol and Roby, 1961). During the course of these investigations it became increasingly clear that any thorough explication of the overall group process of information acquisition depended on understanding the way in which a given team member reacted to information he received from other team members -- particularly in comparison with his reaction to directly observed information.

The specific methodological needs that were pointed up included:

1. A theoretical framework which identifies explicitly the bases for a subject's confidence in his own judgment and his trust in the judgments he obtains vicariously.
2. An experimental paradigm in which these factors can be systematically manipulated or controlled.
3. A theoretical analysis of the dynamic interplay between confidence and trust, and the way in which this may be reflected in overt behavior.
4. A measurement procedure that affords direct evaluation of the net confidence that a subject places in a judgment that is based upon his own opinion and that of a real or fictitious partner.

The present theoretical approach is adapted from a more compre-

hensive treatment of epistemic processes discussed in an earlier report (Roby, in press). The basis for the approach lies in the now familiar concept of "subjective probability" but it employs this concept in a rather special form. Very briefly, it is assumed that subjective probability estimates may be regarded as forming a vector entity (referred to as a 'belief state') which undergoes certain specified transformations under the influence of particular items of external evidence.

The major hurdle in experimental applications of this theoretical framework has been the development of a feasible measure of confidence and trust -- both of which are regarded as probabilistic entities. To show the relation between theory and experiment it will be necessary to describe the former in some detail, even though the present study does not provide direct tests of all of the conjectures here advanced.

Theoretical Framework

The experimental situation that is investigated is one in which the subject makes a series of binary psychophysical judgments with the aid of a simulated partner or "pony," P. The difficulty of the judgments is controlled by varying signal difficulty and the reliability of P is also varied in different experimental conditions. The questions of chief experimental interest are how the subject's confidence in his independent judgment, and trust in P's judgment vary with experimental conditions, and how confidence and trust interact in composite judgments.

It is assumed that, on each judgment trial, the subject will experience some sensory correlate of the external display which will take the form \hat{Y} (yes) or \hat{N} (no). The subject's confidence is defined as the probability he assigns to the event that the external signal, \underline{Y} or \underline{N} , actually corresponds to the sensory correlate. For simplicity, and without serious loss of generality, it will here be supposed that the probability of \underline{Y} and \underline{N} signals, and the associated probabilities of \hat{Y} and \hat{N} sensations, are symmetric. Then confidence is defined by

$$c = \hat{P}(Y/\hat{Y}) = \hat{P}(N/\hat{N})$$

(The probability values are also identified as subjective by the circumflex notation.)

Initial confidence and signal difficulty

Upon initial exposure to a particular judgmental task, the subject does not have any very firm basis for estimating his appropriate confidence level. However, he does have two sources of evidence to go on: the first is his general success in judgmental tasks of the same class; and the second is the internal (central nervous system) distinctiveness of the dif-

ference between \hat{Y} and \hat{N} . Thus it seems quite possible that the subject can make a rough scaling of the distinctiveness of \hat{Y} versus \hat{N} and apply to this an estimate of past judgmental success in dealing with discriminations of similar difficulty.

In an experimental situation the distinctiveness of \hat{Y} or \hat{N} can be manipulated by varying signal difficulty, and the generalized confidence of the subject can be modified by controlling his success or failure experiences. It is assumed that these will interact in a multiplicative fashion to produce an initial confidence value c_0 that is attached to the earliest trial on a new judgmental task. No precise quantitative model will be suggested for c_0 , however, as it is not of direct relevance to the experimental situation here considered.

Modification of c with feedback

If the subject is repeatedly exposed to the judgmental task, receives an internal sensation \hat{Y} or \hat{N} , and has these impressions confirmed or infirmed by trustworthy feedback, his confidence will presumably be modified to conform to his success. Here it will be assumed that this change in confidence from trial to trial is described by a simple operator function. For trials on which the internal impression \hat{Y} or \hat{N} is confirmed, this takes the form,

$$1) \quad c_{n+1} = c_n + \lambda(1-c_n).$$

For those trials on which the internal impression is infirmed by later feedback, the effect is described by,

$$2) \quad c_{n+1} = c_n - \lambda c_n.$$

That is, it is assumed that the effect in either case is to raise or lower the confidence by a fixed proportion, λ , of the possible change in either direction.

Next, the assumption is made that the general expression for all trials will be a weighted combination of the respective effects on success and failure trials. For veridical feedback, there will be a certain proportion d of successful trials, and $(1-d)$ of unsuccessful trials. The proportion d is a direct measure of the subject's accuracy in making the discrimination. Weighting equations 1 and 2 by these proportions, there results,

$$3) \quad c_{n+1} = d[c_n + \lambda(1-c_n)] + (1-d)(1-\lambda)c_n = c_n + \lambda(d-c_n)$$

For the present, interest attaches primarily to the steadystate value of c_n , -- the value at which $c_{n+1} = c_n$. By direct substitution in equation 3

this is seen to occur at the point $c_n = d$. That is, the subject's confidence will asymptotically approach his accuracy of discrimination.

Joint confidence and trust development

A supplementary partner judgment received by the subject may affect his confidence in the judgment on any particular trial as well as his overall confidence level. The latter effect is considered first.

There are four distinct cases: (a) The subject receives veridical feedback to evaluate both his own judgment and that of the partner; (b) The subject receives veridical feedback on his own judgment but not on his partner's (implying that feedback is not received on trials for which the partner's judgment is available); (c) The subject receives veridical feedback on his partner's judgment but not on his own (for the converse reason); and (d) no feedback is received on either his own or the partner's judgment.

For Case (a) it is assumed that trust in the partner's judgment will follow essentially the same operational formula as described by equation 3. If r is the partner's reliability of judgment and t is the trust invested in the partner, then

$$4) \quad t_{n+1} = t_n + \lambda(r - t_n)$$

The asymptotic value for t_n is of course r , the partner's reliability.

In Case (b) the subject has a firm basis for estimating his own accuracy, but can evaluate his partner only by the latter's agreement with his own judgment on no-feedback trials. It is assumed that, on such trials, the subject's trust in his partner is modified by an operator similar to those employed above but weighted by a proportionality factor depending on the subject's own self-confidence. Specifically, it is hypothesized that, for those trials on which S and P agree,

$$5) \quad t_{n+1} = t_n + \lambda(c_n - .5)(1 - t_n)$$

For trials on which they disagree,

$$6) \quad t_{n+1} = t_n - \lambda(c_n - .5)t_n$$

If S and P agree on a proportion g of those trials for which P 's judgment is available, the equations are combined as before, yielding

$$7) \quad t_{n+1} = t_n(1 - \lambda(c_n - .5)) + g\lambda(c_n - .5)$$

Considering only asymptotic values of \underline{t}_n , there are several possibilities for this equation. If \underline{c}_n goes to .5, subsequent values of \underline{t} remain stationary. This outcome is of limited interest. If \underline{c}_n reaches any asymptotic value above .5 then $\underline{t}_{n+1} = \underline{t}_n$ implies $\underline{t}_n = g$. That is, the trust will become identical with the rate of agreement between the subject and \underline{P} .

Case (c) is, of course, the same, mutatis mutandis, as Case (b).

In Case (d), with no feedback at all, there is still the possibility of a sort of bootstrap reinforcement of confidence and trust. This will be based on the inference by \underline{S} that he could agree with the partner on more than a chance number of trials only if they were both performing at better than chance accuracy. Implicit, too, are the assumptions that their judgments are initially independent and that their responses depart from chance in the correct direction. We assume then that the equations for Cases \underline{b} and \underline{c} hold simultaneously. Rearranging them slightly, there results

$$8) \quad \underline{t}_{n+1} = \underline{t}_n + \sqrt{(c_n - .5)(g - t_n)}$$

$$9) \quad \underline{c}_{n+1} = \underline{c}_n + \sqrt{(t_n - .5)(g - c_n)}$$

Subtracting the second equation from the first and rearranging terms,

$$10) \quad \underline{t}_{n+1} - \underline{c}_{n+1} = (\underline{t}_n - \underline{c}_n) \left[1 - \sqrt{(g - .5)} \right]$$

As the term on the right hand side is less than one, it is clear that \underline{t}_n and \underline{c}_n must ultimately become equal.

The specific implication of equation 10 is that

$$11) \quad \underline{c}_n - \underline{t}_n = (\underline{c}_0 - \underline{t}_0) (1 - \sqrt{(g - .5)})^n$$

where \underline{c}_0 and \underline{t}_0 are the initial levels of confidence and trust. But then it follows that $\underline{t}_n = \underline{c}_n - (\underline{c}_0 - \underline{t}_0) (1 - \sqrt{(g - .5)})^n$ which can be substituted into equation 10, giving

$$12) \quad \underline{c}_{n+1} = \underline{c}_n + \sqrt{(c_n - .5)(c_0 - t_0)} (1 - \sqrt{(g - .5)})^n (g - c_n).$$

This equation is non-linear with a variable coefficient so that explicit solution is difficult. However, it can be solved for the asymptotic value \underline{c} . The roots of the equation $\underline{c}_{n+1} - \underline{c}_n = 0$ occur at $\underline{c}_n = .5$ and $\underline{c} = g$. The latter root is assumed to represent the typical outcome: both confidence and trust become equal to the rate of agreement.

Composite judgments

The other aspect of the relation between \underline{c} and \underline{t} is their interaction

on a single trial. Given a specified level of \underline{c} and \underline{t} on a particular trial and a certain internal sensation, \hat{Y} or \hat{N} , how will the subject utilize a supplementary partner judgment that may agree or disagree with his own?

The appropriate formula for combining the values for \underline{c} and \underline{t} in order to estimate composite confidence, \underline{v} , would seem to have the following properties:

- (1) The direction of the final judgment, \underline{Y} or \underline{N} , should be the same as that of the judgment associated with the greater of \underline{c} or \underline{t} .
- (2) \underline{v} should be greater than \underline{c} if the subject and \underline{P} agree, but less if they disagree.
- (3) If either \underline{c} or \underline{t} is at .5, it will not affect \underline{v} at all.
- (4) As either \underline{c} or \underline{t} approaches 1.0, so will \underline{v} .

These criteria suggest that \underline{c} and \underline{t} should be combined by the standard Bayes formula for calculation of inverse probability. Thus if the subject's own initial judgment is that the signal is present, and the partner agrees

$$13) \quad v_1 = ct/(ct + \bar{c}\bar{t}) = ct/g$$

If they disagree, the expression is

$$14) \quad v_2 = c\bar{t}/(c\bar{t} + \bar{c}t) + \bar{c}\bar{t}/\bar{g}$$

assuming that \underline{c} is greater than \underline{t} .

Verbally, equation 13 may be paraphrased from the subject's standpoint as, "the conditional probability that the joint judgment (\underline{Y} or \underline{N}) is correct, if both \underline{P} and I think it is correct, is the probability of our agreeing on a correct judgment divided by the total probability of agreement."

Empirical derivation of confidence and trust indices

In practice it may not be possible to measure \underline{c} and \underline{t} directly, and will be necessary to infer those quantities from observed values of the composite confidence, \underline{v} . This section will derive equations from which the estimate may be made.

Suppose that the subject's composite confidence on those trials in which he is in agreement with \underline{P} is \underline{v}_1 and that the composite confidence is \underline{v}_2 on trials in which the subject and \underline{P} disagree. Then from earlier results, it is assumed that

$$15) \quad v_1 = ct/(ct + \bar{c}\bar{t})$$

$$16) \quad v_2 = \bar{c}\bar{t}/(\bar{c}\bar{t} + ct)$$

15 is solved to obtain an expression for \underline{t} , i.e.,

$$17) \quad t = \bar{c}v_1 / (c\bar{v}_1 + \bar{c}v_1)$$

from which also

$$18) \quad \bar{t} = c\bar{v}_1 / (c\bar{v}_1 + \bar{c}v_1)$$

These values are then substituted in the second equation, giving

$$19) \quad c\bar{v}_2 \cdot c\bar{v}_1 / (c\bar{v}_1 + \bar{c}v_1) = \bar{c}v_2 \cdot \bar{c}v_1 / (c\bar{v}_1 + \bar{c}v_1)$$

$$19a) \quad c^2 v_2 \bar{v}_1 = \bar{c}^2 v_1 v_2$$

This can be solved in turn to provide a quadratic expression for \underline{c} , i.e.,

$$20) \quad c = \frac{v_1 v_2 \pm \sqrt{v_1 v_2 v_1 v_2}}{v_1 + v_2 - 1}$$

and \underline{t} can thus be found by substitution in one of the earlier equations.

To illustrate, suppose that $\underline{v}_1 = .80$ and $\underline{v}_2 = .60$. Then

$$c = \frac{.48 \pm \sqrt{.0384}}{.40} = \frac{.48 \pm .196}{.40} = 1.69 \text{ or } .71$$

The latter value is obviously the appropriate one. Then, from equation 17,

$$t = .29 \times .80 / (.29 \times .80 + .71 \times .20) = .62$$

Putting these values back in equation 15 and 16 gives

$$\underline{v}_1 = .71 \times .62 / (.71 \times .62 + .29 \times .38) = .80 \text{ as obtained}$$

$$\underline{v}_2 = .71 \times .38 / (.71 \times .38 + .62 \times .29) = .60 \text{ as obtained}$$

Measurement of confidence

In order to test the consequences of these formulations, it is necessary to have a sensitive and valid measure of confidence and associated constructs. A direct introspective report of confidence -- e.g. simply asking the subject "how sure he is" of his judgment -- has obvious drawbacks. In particular, the results obtained under these instructions must depend upon whether the subject interprets his objective as maximizing the stated confidence on successful trials or minimizing the stated

confidence on unsuccessful trials.

Even the device of rewarding the subject for the realism of his estimated confidence is of questionable value. The basic difficulty here is that if the reward is based on the overall agreement between mean confidence and mean accuracy it does not provide an incentive for accurate estimation of confidence on each trial. The subject's best strategy is to report a fixed confidence value that accords with his rate of success, ignoring trial-to-trial fluctuations in subjective confidence.

The measure here suggested, and incorporated in the empirical study, is based on the more general conceptual framework cited earlier (Roby, 1962). This measure offers the subject a graded family of bets with payoffs dependent upon stated confidence. Specifically, the payoff for a correct judgment is proportional to $\underline{c}/\sqrt{\underline{c}^2 + \bar{c}^2}$ and the payoff for an incorrect judgment is $\bar{c}/\sqrt{\underline{c}^2 + \bar{c}^2}$. Thus the potential gain on a successful trial increases as the stated \underline{c} increases, but the gain for an unsuccessful trial decreases. Because the denominator increases from a minimum at $\underline{c} = \bar{c} = .5$ to a maximum value at $\underline{c} = 1$, the subject is penalized for overstating his confidence.

As a numerical illustration, suppose that the subject's actual confidence is .75 that the signal is present. The corresponding optimal bet is (.75, .25) with payoffs of $.75/\sqrt{.75^2 + .25^2} = .948$ and $.25/\sqrt{.75^2 + .25^2} = .316$ if the signal is present or absent, respectively. The expected payoff is $.75 \times .948 + .25 \times .316 = .790$. If the subject selects the bet corresponding to (.80, .20) then the expected payoff is $(.75 \times .80 + .25 \times .20)/.824 = .7888$. If he selects the more conservative bet corresponding to (.70, .30) the expected payoff is $(.75 \times .70 + .25 \times .30)/.761 = .7884$. Although the loss is not great for a discrepancy of this magnitude, it increases rather sharply for more inappropriate bets.

Experimental Procedure

The judgmental task on which confidence measures were obtained entailed a discrimination between a standard angle of 90° , and a comparison angle of less than 90° . The angles were printed on cards and displayed in an illuminated aperture. On each trial, the subjects were shown a card which might contain either the 90° standard or the comparison angle. They were required to report their judgment within a fixed time interval.

The response consisted in setting a movable pointer along a linear scale from +10, corresponding to virtual certainty that the display was the 90° angle, to -10 representing virtual certainty that it was not the 90° angle. The subject indicated that he had made his final adjustment for a trial by pressing a test button. Under feedback conditions this test resulted in an indication to the subject of his earnings on that trial. Under

the no-feedback condition, the display meter, which showed trial-to-trial earnings, was covered.

For future reference, Table 1 contains a summary of the various response measures and corresponding payoffs. The "recorded position" was the pointer position that was indicated to the experimenter and became the basic raw data of the study. The "dial position" was the setting on the subject's response panel; unlike the recorded position, it was explicitly directional in order to emphasize the amplitude of the response away from the neutral position. The "angle equivalent" column divides the 20 pointer positions into equal intervals of 4.74° spanning the range from 0° to 90° , with 45 corresponding to the neutral position.

The two "payoff" columns are symmetrical with respect to the neutral position. The numbers appearing in these columns are obtained by taking sines and cosines, respectively, of the angle equivalents, subtracting .707, the sine or cosine of 45° , and multiplying by a factor of ten. The "probability equivalents," finally, are equal interval divisions of the 20 steps from 0-100% probability. These values are used for all comparisons with accuracy, and for other computational purposes.

Experimental design

The overall experimental design is given in Table 2. The main variables reflected in this design are:

a) Feedback - one group of 16 subjects was run with feedback on all trials: that is, after each judgment they were given an indication of their earnings on that trial. A second group of 16 subjects was run in an experimental design that was identical except that they were not given feedback.

b) Blocks of trials - each subject had 5 blocks of 50 trials each, all completed in a single experimental session. The first block, for all subjects, was without any hypothetical partner; in the four succeeding blocks of trials, they were given a supplementary partner report or "pony" on each trial.

c) Signal difficulty - two levels of signal difficulty were introduced by setting the size of the comparison angle at 89° for the hard discrimination, and 87° for the easy discrimination. Earlier results had indicated that correct detection in the former case occurred about 65 % of the time and in the latter case about 85% of the time.

d) Pony reliability - The subjects were told that the supplementary reports they would receive were not completely reliable and were advised of the actual reliability levels -- 64% and 84% respectively -- for two reliability levels. These levels were selected to correspond to the signal difficulty values, modified only for rounding errors. There were exactly 16 correct answers in each set of 25 trials for the 64% pony, and

exactly 21 correct answers in 25 trials for the 84% pony.

The chief between-subject variable (other than feedback) was the order in which they had various combinations of signal difficulty and pony. The basic design is a four-by-four latin square in which the four signal-pony combinations occur in each serial position. In addition, however, within each of the subgroups of four subjects who had the same sequence, two subjects had the hard discrimination in the initial trial block, and two subjects had the easy discrimination. These trial blocks of course occurred before the pony was introduced.

e) List - the blocks of 50 trials consisted of 25 actual signal trials and 25 actual no-signal trials. The order of signal-no-signal occurrence was randomized 16 times to generate 16 distinct lists. Within the main sequence of trial blocks (i.e., the last four) each subset of four subjects had all of the 16 lists, and the subjects did not have the same list twice.

Procedure

Subjects were male and female Tufts undergraduates. They were paid at an hourly rate for participating in the study and also were told that a \$5.00 reward would be given for best performance.

The instructions (Appendix) explained the judgmental task and the reward or payoff system. They were given to the subjects to read, and E answered relevant questions.

At the beginning of the experiment, and before each of the remaining four blocks of 50 trials, the subject had a 10 trial procedural "warmup." On these 10 trials, the signal and pony were presented as for regular trials and the subject had an opportunity to respond as he would on actual trials. These data were recorded but will not be reported.

After the subject had made each judgment by positioning the response switch and testing, he also wrote down his response and the resulting payoff (under feedback conditions). The payoffs were totalled by the subjects after 25 trials.

Subjects were also given a brief questionnaire asking how much they used the pony. These data are not analyzed in the present report.

Results¹

Confidence and accuracy on pre-pony trials

The first set of data describe the performance of subjects on the 50 trials prior to introduction of the pony. On these trials half of the subjects had the 87° (easy) discrimination and half had the 89° (difficult) discrimination. The chief results are summarized in Table 3. They are classified in terms of the discrimination angle. Trials are broken down into first 25 and second 25. Feedback and non-feedback subjects are separated; and the confidence measures are separately averaged for those trials on which the subjects were correct and those trials on which the subjects were incorrect. The following conclusions appear to be justified:

1. There is no appreciable improvement in detection in the first to the second sets of 25 trials. This holds for both feedback and non-feedback subjects.
2. Feedback subjects do not perform better than non-feedback subjects in terms of accuracy.
3. Subjects perform better on the 87° signal than the 89° signal. The pooled accuracy for the former is .751 and the latter is .624. These rates are slightly lower than were obtained on pilot studies but still well enough separated to serve the main purposes of the study.
4. Confidence scores are higher for the feedback subjects than for the non-feedback subjects for the easy angles but not for the hard angle discrimination.
5. Confidence is uniformly higher for the 87° angle than for the 89° angle for the FB subjects. This does not hold however for the \overline{FB} subjects.
6. The confidence scores are higher when subjects are correct than when the subjects are incorrect.

This last result is shown in more detail in a slightly different way in Table 4. This table presents the relation between accuracy and each of the dial settings corresponding to a confidence level. The first column gives the dial setting from 1 to 10 which the subjects adjusted on each trial. The next four columns give the corresponding accuracy proportions at that setting followed by the mean accuracy across all subjects and all angles, and the final column gives the equivalent confidence level, that is the accuracy setting transformed into the corresponding proportion.

Although the relation between accuracy and confidence is monotonic within sampling error (the product-moment correlation of the unweighted

¹ Some of the statistical analyses reported here were completed at the Massachusetts Institute of Technology Computations Laboratory.

scores is .819), it is clear that the confidence tends to be exaggerated at the higher level. As evidence, the regression between confidence and accuracy is given by the formula $c = .062 + 1.28a$. As noted earlier FB subjects tend to be more overconfident at the 87° level and there is a slight tendency for the FB subjects to be more overconfident at the 89° level. These data and the result noted above (that subjects are more confident when they are correct) do not show clearly whether the effect is due to within-subject or between-subject differences; that is, they do not show whether the individual subject is more likely to give a high confidence response on those trials on which he is correct or whether it is simply true that more accurate subjects use higher confidence responses across the board. Later results give sharper but still not conclusive evidence on this point.

Accuracy and confidence with pony available

Table 5 contains the accuracy scores under the various experimental conditions for the final four sessions after the pony was introduced. The scores for the first and second 25 trials showed no differences, as before, and have been pooled in this table. The accuracies are computed separately for those trials in which the pony was correct and the trials on which the pony was incorrect, and the weighted means over both correct and incorrect trials are also presented.

For these data as before there are no differences between the FB and FB subjects, and the accuracies are clearly greater for the 87° discrimination than for the 89° discrimination. The interesting result here is the interaction effect between pony reliability and correctness. For the more reliable 84% pony, the subjects' composite judgments are more correct when the pony is correct, but less accurate when the pony is wrong. The natural interpretation of this is that the subjects tended to depend on the reliable pony more than the less reliable pony. At least for the easy discrimination, however, the net result is that the subjects do little better when they have the pony answer to lean on. As the judgment becomes more difficult, the pony becomes more helpful; but it should be noted that only in the case of the easy angle and the 64% pony does the subject do better than he would by following the pony exactly.

In Table 6 the mean confidence scores are given for each of the eight main experimental conditions and, within conditions, for each of the four event conditions defined by the correctness of the subject and agreement with the pony. As before, the confidence values have been converted to probability for purposes of comparison and as before they tend to be high relative to the accuracy under corresponding conditions. The almost uniform result shown in this table is that subjects are more confident, whether they are correct or incorrect, when they agree with the pony. The sole discrepancy is for the easy 87° discrimination with the less re-

liable 64% pony. It also tends to be true for the 87° discrimination that subjects are more confident when they are correct than when they are incorrect. This last result will be examined again in greater detail in a later section.

Derived c and t indices

These results are put in much sharper focus if the composite confidence scores are used to derive estimates of confidence in the subjects' judgment (independent of the pony's answer) and trust in the pony as discussed in the introductory section. The values for v_1 are the mean probabilities ascribed to the signal being present when it is present and the pony is correct or to the signal not being present when it is not present and the pony is correct. The values of v_2 are the corresponding probabilities when the pony's response is incorrect; thus, for example a composite confidence of .70 attached to an incorrect judgment is treated as a "correct" judgment with the composite confidence of .30. Results given in Table 7 are divided into estimates for the FB and $\overline{\text{FB}}$ conditions but also subjects within both FB and $\overline{\text{FB}}$ conditions have been divided into two groups. The top set of means in each case refer to subjects who tended to distribute their confidence values in the uni-modal way assumed in the computational scheme. The lower means in each case describe the confidence scores for the subjects who tended to "over-use" the extreme confidence values. Thus the top scores are perhaps on somewhat firmer ground. However the conclusions listed below follow for both sets of scores:¹

1. Confidence is greater for the 87° judgment than for the 89° judgment (This result holds for 31 of 32 sign-test comparisons in the original data.)
2. The trust is greater for the 84% pony than for the 64% pony.
3. Confidence exceeds trust for the 87° judgment (in 27 of 32 sign-test comparisons) and is lower than trust for the 89° judgment (in 24 of 32 sign-test comparisons).

These results are all in accordance with common sense expectations and tend to vindicate the rather tenuous chain of inference upon which these indices are based.

¹This procedure is based on the assumption that the internal signal associated with the objective presence of the signal has a continuous uni-modal distribution about a positive value but running through the 50% point. This choice of assumptions was dictated by parsimony and computational convenience. Subsequent analysis indicates that it results in an underestimate of both \underline{c} and \underline{t} values, but that the relative magnitudes are maintained without great distortion.

Variance estimates for unfolded confidence scores

Although it did not seem judicious to depend heavily on parametric statistics for this exploratory study, an analysis of variance was performed in an effort to determine the relative magnitudes of separate sources of variation. Table 8 gives certain sums of squares extracted from a 2⁷ factorial analysis of variance of the composite confidence scores classified in terms of all experimental and event conditions. The scores considered here are the "unfolded" or full range scores running from 1 to 20 which in fact reflect both confidence and accuracy. These results and the associated mean differences (which will be converted to p values) support the following conclusions:

1. There is no bias in favor of signal presence or absence; the mean confidence over all events and conditions is almost exactly 50%.
2. All experimental treatments combined account for about half the variance in the composite judgment. Both subject and treatment effects are highly significant as compared with their interaction.
3. There is a slight but significant tendency (indicated by item c in the table) for a composite judgment in favor of signal presence under the difficult discrimination.
4. Signal presence accounts for about half of the total variation due to treatments; of course, with perfect discrimination this would be the sole source of variation. The mean confidence scores with signal present and absent are .642 and .358 respectively.
5. The second largest source of variation is the interaction between signal difficulty and the presence or absence of the signal on a particular trial. With the difficult 89° test angle and the signal present the mean confidence is .573; with the easy 87° test angle and the signal present it is .710.
6. Availability of feedback interacts with both the signal and pony events as shown by the interactions a x f and a x g. These results are summarized by the statement that FB subjects are influenced somewhat more by the signal and somewhat less by the pony.
7. The significant interaction (cxg) between the test angle and the pony event indicates the greater influence of the pony when the discrimination is difficult.
8. The interaction (dxg) between the pony reliability and pony event (That is "yes" or "no" pony answer on a particular trial) is, as would be expected, reflected in a greater influence of the 84% than of the 64% pony.

Thus the analysis of variance results, here treated as only descriptive material, do tend to corroborate the results obtained on the more conservative sign-tests presented earlier. They also give some preliminary indication of the relative magnitudes of the effects in quantitative terms.

Confidence and Accuracy

This section will return to the relation between confidence and accuracy and in particular to the within-subject aspect of this; that is, not just whether the accurate subjects tend to be more confident, but also whether individual subjects tend to be more confident on trials upon which they are correct. In order to investigate this, the mean confidence scores (folded) have been computed for each of the subjects under each of the four experimental conditions and each of the four event conditions defined by the subject's correctness and the subject and pony correctness. The data here considered as before are the sign tests, in this case the signs of the differences between the means when the subject is in fact correct and the mean confidence when the subject is incorrect, controlling for both experimental and event conditions.

As far as it goes, the evidence is quite clear that individual subjects do make more confident judgments when they are correct. In a total of 384 sign test comparisons 231 or 60% are in favor of higher confidence on correct trials all other conditions being identical. A more detailed analysis shows, however, that this is due almost entirely to those trials on which the pony is also correct. Under those conditions the confidence is higher for the correct judgment in 73% of all cases whereas it is higher on only 44% of the cases in which the pony is incorrect. This leaves the question of individual reaction to correctness still up in the air.

It might be noted incidentally that there is no evidence in the data described above for any effect due to feedback or to acquisition over successive sessions; that is, there is no tendency for subjects to become more realistic in their estimates due to either of these learning conditions.

Taking one more step along this line, the final result concerns the relation between the actual point gains of subjects and the score they might have achieved with a more realistic appraisal of accuracy of judgment. The latter value is computed by assuming that the "ideal subject" would on each trial use the confidence level corresponding to his mean accuracy over all trials under that particular experimental condition.

The results in terms of discrepancies bear out the following points:

1. In general subjects do not score as well as they might by selecting a uniform confidence for all trials in accordance with their long run accuracy. Negative discrepancy scores are obtained by 24 of the 32 subjects and in 83 of the 128 sessions. Moreover the negative discrepancies are in general considerably larger than the positive ones.
2. There is a correlation between negative discrepancy and low accuracy; that is, subjects tend to lose potential points under poor performance. At the same time it should be mentioned that the mean loss per trial due

to unrealistic confidence level is only .20 points on the 10 point scale; thus, it appears that subjects do compensate by high confidence bets on trials on which they are in fact correct.

3. There is no relation in these data with either feedback or with training; that is, there is no evidence of learning to become more realistic over trials.

It must then be concluded that these data do not provide a firm answer to the question of whether the subjects are sensitive to fluctuations in soundness of judgment from trial to trial. Some additional evidence might have been obtained from the pre-pony trials but it seemed better advised to reserve this question for a later study.

Summary

From a methodological standpoint the results of this report are distinctly encouraging. The experimental manipulations for the most part operate as expected, and the response measures appear to be sensitive to experimental conditions.

At the same time, it is clear that several further methodological improvements are required before definitive results can be obtained.

1. Although the subjects in general appeared to use the 'betting' scheme in a rational (gain maximizing) way, many subjects still used the extreme scores with inordinate frequency. In order for the measurement procedure here employed to be fully successful this "sporting" reaction must be further minimized. In research subsequent to that reported here, an attempt has been made to achieve this by identifying the confidence judgments directly with probability rather than using the 10 point scale. If this and related instructional devices are unsuccessful, the rather unpalatable alternative would be to discard or isolate non-conforming subjects on the basis of score distributions.

2. At no point in the above results does the effect of the availability of trial-to-trial feedback seem to be as pronounced as one would expect. There are few major differences between FB and FB subjects, and little evidence of acquisition on the part of the latter subjects. One suggested procedural modification would be to provide FB subjects with a cumulative record of earnings as well as feedback on each trial. One of the followup studies mentioned below should shed more light on the desirability of such modification.

Appendix

Instructions

You will be shown angles, some of which are 90° , i.e. right angles, and some of which are smaller. Your task will be to judge whether the angle you see on any given trial is a right angle. In addition, we want to know how sure you are of your decision. Therefore, we will allow you to bet up to 10 points on your judgments.

The angles will appear behind this glass frame when the light goes on. You will see the angle for half a second and then have approximately 15 seconds in which to make your bet. Use the pointer on the scaled panel in front of you to indicate the bet. Notice that the scale goes from a maximum number of 10 points on the left for a "no" to a maximum number of 10 points on the right for a "yes." Do not use the zero point at the center since this means that you are unwilling to bet; and we want you to make a bet, however small, in one direction or the other. After you have placed your bet, you may find out whether you have won or lost points. To do this, push the button to the right of the pointer scale and read the results from the meter facing you. A negative value indicates a loss; and a positive one, a gain. With respect to payoffs on your bets, keep in mind that as the bet size increases, the size of losses increases at a more rapid rate than does the size of the gains. I will illustrate this to you when you have finished reading the instructions. Every 25 trials you will be given two minutes to figure out your earnings.

Here is exactly how a trial will proceed.

1. You see the angle for half a second.
2. You decide what you want to bet on your decision, set the pointer at the location corresponding to your bet, and enter the amount of the bet in the proper column on the sheet in front of you, i.e., the "Yes" or the "No" column, depending on your decision.
3. Push the button to the right of the pointer, indicating to me that you are "testing." Read the payoff. Enter the payoff in the proper column, i.e. "+ column" if you have won points and "-column" if you have lost points. Reset the pointer to zero. Please try to keep your record neat and easily readable.

You will see 5 sets of 50 angles each. In each set there will be 25 right angles and 25 smaller angles. For the first set the procedure will be exactly as outlined above. For the other 4 sets you will have additional information which may help you in making decisions. I have a list of a hypothetical subject's responses to the same sets of angles I will

be showing you, presented in the same order as you will be seeing them. For 2 of the sets he responds correctly 64% of the time and for the other two, 84% of the time. Before beginning each set I will tell you what this percentage is; and before each trial I will tell you what the hypothetical subject's response is for that trial.

Your object throughout the experiment will be to win as many points as possible. The person who gets the highest number of points will get \$5.00 in addition to what he earns for working as a subject.

Keep in mind the fact that there is no patterned order of presentation in any set of angles and that for each set of 50, there will be 25 right angles and 25 that are smaller. For each set the smaller angle is the same size; and I will tell you before beginning the set exactly what the size of the smaller angle will be. If you have any questions, please ask them before we begin the experiment because after that we will be working fairly rapidly and without interruption.

Table 1

Correspondences between Confidence Measures
and Payoff Values

| <u>Recorded position</u> | <u>Dial position</u> | <u>Angle equivalent</u> | <u>Payoff Yes</u> | <u>Payoff No</u> | <u>Percent equivalent</u> |
|------------------------------|--------------------------|-----------------------------|-----------------------|----------------------|-------------------------------|
| 1 | 10 no | 0 | -7.07 | 2.93 | 0 |
| 2 | 9 " | 4.74 | -6.14 | 2.90 | 5.26 |
| 3 | 8 " | 9.47 | -5.42 | 2.79 | 10.52 |
| 4 | 7 " | 14.21 | -4.62 | 2.62 | 15.78 |
| 5 | 6 " | 18.95 | -3.81 | 2.38 | 21.04 |
| 6 | 5 " | 23.68 | -3.06 | 2.09 | 26.30 |
| 7 | 4 " | 28.42 | -2.31 | 1.80 | 31.56 |
| 8 | 3 " | 33.16 | -1.60 | 1.30 | 36.82 |
| 9 | 2 " | 37.89 | -0.94 | 0.72 | 42.08 |
| 10 | 1 " | 42.63 | -0.30 | 0.28 | 47.34 |
| 11 | 1 yes | 47.37 | 0.28 | -0.30 | 52.60 |
| 12 | 2 " | 52.10 | 0.72 | -0.94 | 57.86 |
| 13 | 3 " | 56.84 | 1.30 | -1.60 | 63.12 |
| 14 | 4 " | 61.58 | 1.80 | -2.31 | 68.38 |
| 15 | 5 " | 66.31 | 2.09 | -3.06 | 73.64 |
| 16 | 6 " | 71.05 | 2.38 | -3.81 | 78.90 |
| 17 | 7 " | 75.79 | 2.62 | -4.62 | 84.16 |
| 18 | 8 " | 80.52 | 2.79 | -5.42 | 89.42 |
| 19 | 9 " | 85.26 | 2.90 | -6.14 | 94.68 |
| 20 | 10 " | 90.00 | 2.93 | -7.07 | 100.00 |

Table 2

Experimental Design

BLOCK OF 50 TRIALS

| | | BLOCK OF 50 TRIALS | | | | |
|-------|----|--------------------|----------------|-------|-------|-------|
| | | 1 | 2 | 3 | 4 | 5 |
| Group | S | < | | | | |
| 1 | 1 | H | \overline{P} | | | |
| | 2 | H | | H 64% | H 84% | E 64% |
| 2 | 3 | E | | | | E 84% |
| | 4 | E | | | | |
| 3 | 5 | H | \overline{P} | | | |
| | 6 | H | | H 84% | H 64% | E 84% |
| 4 | 7 | E | | | | E 64% |
| | 8 | E | | | | |
| 5 | 9 | H | \overline{P} | | | |
| | 10 | H | | E 64% | E 84% | H 64% |
| 6 | 11 | E | | | | H 84% |
| | 12 | E | | | | |
| 7 | 13 | H | \overline{P} | | | |
| | 14 | H | | E 84% | E 64% | H 84% |
| 8 | 15 | E | | | | H 64% |
| | 16 | E | | | | |

H: 89°

E: 87°

Table 3

Accuracy and Confidence Scores Under Various Experimental
Conditions Before Introduction of Pony

| Angle | | 1st 25 Trials | | 2nd 25 Trials | |
|-------|---------------------|-------------------|-------------------|-------------------|-------------------|
| | | <u>Proportion</u> | <u>Confidence</u> | <u>Proportion</u> | <u>Confidence</u> |
| 87° | <u>FB</u> - Correct | 77.5 | 81.5 | 73.5 | 83.0 |
| | - Incorrect | 22.5 | 73.6 | 26.5 | 75.3 |
| | Pooled | -- | 79.7 | -- | 80.9 |
| | FB - Correct | 72.5 | 85.4 | 77.0 | 91.3 |
| | - Incorrect | 27.5 | 81.6 | 23.0 | 82.7 |
| | Pooled | -- | 84.4 | -- | 89.3 |
| 89° | <u>FB</u> - Correct | 60.0 | 82.1 | 67.5 | 84.1 |
| | - Incorrect | 40.0 | 80.9 | 22.5 | 80.0 |
| | Pooled | -- | 81.6 | -- | 82.8 |
| | FB - Correct | 65.5 | 79.0 | 56.5 | 83.5 |
| | Incorrect | 34.5 | 75.2 | 43.5 | 81.2 |
| | Pooled | -- | 77.7 | -- | 82.4 |

Table 4

Detection Accuracy at Various Levels of Confidence
Under Differing Experimental Conditions

| Dial Setting | <u>87°</u> | | <u>89°</u> | | <u>Subjects'</u> <u>Mean</u> <u>Accuracy</u> | <u>Equivalent</u> <u>Confidence</u> <u>Level</u> |
|--------------------|------------|-----------|------------|-----------|--|--|
| | <u>FB</u> | <u>FB</u> | <u>FB</u> | <u>FB</u> | | |
| 1 | .50 | .00 | .40 | .40 | .44 | .53 |
| 2 | .65 | .55 | .62 | .69 | .64 | .58 |
| 3 | .75 | .60 | .67 | .53 | .61 | .63 |
| 4 | .69 | .78 | .57 | .64 | .63 | .68 |
| 5 | .60 | .67 | .58 | .61 | .58 | .74 |
| 6 | .67 | .55 | .58 | .60 | .59 | .80 |
| 7 | .78 | .69 | .63 | .65 | .69 | .84 |
| 8 | .88 | .75 | .61 | .67 | .78 | .89 |
| 9 | .77 | .74 | .93 | .52 | .73 | .95 |
| 10 | .86 | .84 | .68 | .69 | .77 | 1.00 |
| Mean Accuracy | .755 | .747 | .637 | .610 | | |
| Mean confidence | .800 | .868 | .822 | .786 | | |

Table 5

Mean Accuracy of Composite Judgments Under
Various Experimental Conditions

| | | <u>No Feedback</u> | | | <u>Feedback</u> | | |
|-----|-----|-------------------------|---------------------------|-----------------|-------------------------|---------------------------|-----------------|
| | | <u>Pony Correct</u> | <u>Pony Incorrect</u> | <u>Combined</u> | <u>Pony Correct</u> | <u>Pony Incorrect</u> | <u>Combined</u> |
| 87° | 64% | .833 | .775 | .812 | .800 | .720 | .771 |
| 87° | 84% | .845 | .656 | .815 | .843 | .628 | .808 |
| 89° | 64% | .645 | .494 | .591 | .641 | .550 | .608 |
| 89° | 84% | .781 | .312 | .706 | .711 | .463 | .671 |

Table 6

Mean Confidence (Corrected to Probability) Under
Various Experimental and Event Conditions

| <u>Angle Diffi- culty</u> | <u>Pony- Relia- bility</u> | | <u>No Feedback</u> | | <u>Feedback</u> | |
|-----------------------------------|------------------------------------|-----------|-----------------------------|--------------------------------|-----------------------------|--------------------------------|
| | | | <u>Agrees With Pony</u> | <u>Disagrees With Pony</u> | <u>Agrees With Pony</u> | <u>Disagrees With Pony</u> |
| 87° | 64% | correct | .866 | .824 | .901 | .894 |
| | | incorrect | .777 | .763 | .827 | .852 |
| 87° | 84% | correct | .880 | .829 | .904 | .880 |
| | | incorrect | .805 | .769 | .871 | .816 |
| 89° | 64% | correct | .814 | .778 | .846 | .830 |
| | | incorrect | .810 | .790 | .858 | .780 |
| 89° | 84% | correct | .863 | .776 | .852 | .840 |
| | | incorrect | .891 | .789 | .834 | .797 |

Table 7

Mean Estimated Confidence (c) and Trust (t)
Under Various Experimental Conditions

| <u>Signal Difficulty</u> | <u>Pony Reliability</u> | <u>Feedback Conditions</u> | | <u>No Feedback Conditions</u> | |
|------------------------------|-----------------------------|--------------------------------|----------|-----------------------------------|----------|
| | | <u>c</u> | <u>t</u> | <u>c</u> | <u>t</u> |
| 89° | 64% | .549 | .553 | .542 | .537 |
| | | .594 | .546 | .545 | .575 |
| 89° | 84% | .554 | .623 | .506 | .643 |
| | | .577 | .573 | .429 | .785 |
| 87° | 64% | .692 | .569 | .647 | .533 |
| | | .792 | .510 | .820 | .565 |
| 87° | 84% | .699 | .591 | .643 | .574 |
| | | .787 | .568 | .783 | .634 |

Table 8

Variance Associated With Selected Combinations
of Experimental and Event Conditions

| <u>Source</u> | <u>df</u> | <u>Sums of Squares</u> | <u>F</u> |
|----------------------|-----------|------------------------|----------|
| a) feedback | 1 | .02 | .00 |
| b) pretraining angle | 1 | 1.95 | .13 |
| c) test angle | 1 | 77.21 | 5.22 |
| d) pony reliability | 1 | 1.85 | .12 |
| e) trials | 1 | 4.36 | .29 |
| f) signal presence | 1 | 7300.30 | 493.35 |
| g) pony answer | 1 | 2284.71 | 154.40 |
| a x f | 1 | 83.76 | 5.66 |
| a x g | 1 | 107.30 | 7.25 |
| b x g | 1 | 125.57 | 8.49 |
| c x f | 1 | 2541.27 | 171.74 |
| c x g | 1 | 183.80 | 12.42 |
| d x g | 1 | 411.31 | 27.80 |
| All treatments | 127 | 14650.00 | 7.80 |
| Subjects | 7 | 256.71 | 2.48 |
| Treatments x Ss | 889 | 13154.78 | |

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| <p>This report is concerned with the development of a research methodology and a theoretical framework for investigating the effects of social influence in a simple judgmental situation. The laboratory task entails a simple binary judgment as to whether a displayed angle departs from 90°; before making his own response the subject is provided with the answer of a hypothetical partner, programmed at a certain fixed accuracy level. The responses are made in terms of a special betting scheme which penalizes the subject for overstating or understating his confidence. The two main experimental variables in this study are the difficulty of the discrimination and the announced reliability of the hypothetical partner. Theoretical predictions as to the effects of these variables on the relative value of confidence measures are confirmed. However, further methodological development is required to increase the realism of subjects' confidence scores.</p> | | | |

14.

KEY WORDS

LINK A

LINK B

LINK C

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 Human Behavior
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